

OWEN, HUNTER AND HARVEY.

(3)

A LETTER

TO THE

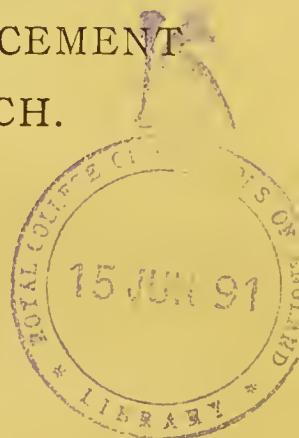
PRESIDENTS AND COUNCIL

OF THE

ASSOCIATION FOR THE ADVANCEMENT
OF MEDICINE BY RESEARCH.

FROM THE AUTHOR OF

"HUNTER AND THE STAG."



"Detractors, mummers and writers defiled with abuse, as I resolved with myself never to read them, satisfied that nothing solid or excellent, nothing but foul terms, was to be expected of them, so I have held them still less worthy of an answer."—*Circulation of the Blood*, by W. HARVEY, M.D.

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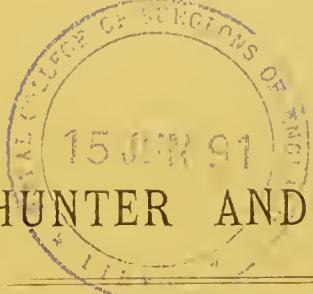
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OWEN, HUNTER AND HARVEY.



Before proceeding to the consideration of the second case put forward by Professor Owen, in confutation of my remark that "Vivisection, while it panders to scientific curiosity, adds nothing to practical knowledge" a few lines are needful as to the issue already raised. Fortunately very few will suffice.

And first, a word, not exactly of apology but of explanation. Knowing what I now know I cannot but see that the personal appeal with which I concluded my former letter must have worn in Mr. Owen's, and possibly in other eyes, an air of irony.* Nothing could be

* The following is the passage in question:—"I leave it to you, Sir, as a man who, whatever his devotion to 'science,' is not, I hope prepared, as too many of the common crowd of scientists would seem to be, to sacrifice upon its altar his own candour, honour, and self-respect, to say frankly how far I have proved, or failed to prove, my point. . . .

"We, opponents of Vivisection, are taunted everywhere with ignorance of our subject and incapacity for arguing it. Its advocates are never weary of asserting that the facts and reason of the case are alike wholly on their side. But never yet has a too great confidence in their cause betrayed them even for a moment into the weakness of openly discussing any of the points on which their position is so unimpeachable or of meeting argument with any riskier answer than assertion and abuse. I venture, Sir, to hope that in this, as in so many other respects, you will show your superiority to your colleagues. I offer you, with all respect, a scientific argument. Meet it I pray you, as it should be met by a man of science. . . . Do not condescend to that 'policy of silence' which, however befitting to the *Lancet* and the *British Medical Journal* is surely unworthy of one who occupies a position and bears a reputation such as yours."

further from my intention. The appeal was made in perfect good faith, in entire ignorance, or absolute forgetfulness, of the scientific scandals of a period during which my attention was taken up by quite other matters. At its result, as shown in Mr. Owen's article in the *Nineteenth Century* no one could have been more naïvely astonished than myself.

Dealing next with that article I find that Mr. Owen's reply to my argument is threefold.

1. He represents me as stating that "Hunter merely adopted the plan of ligature previously in use by a French surgeon." Now the answer to this is brief—so brief, that but for convention's sake it might be monosyllabic. It is a mis-statement pure, simple and deliberate. Not only untrue but the plain reverse of truth. So far from maintaining any such proposition as it attributes to me the whole argument of the pamphlet in question is based from first to last upon its negation.

2. Mr. Owen then falls back upon a simple re-assertion of his original position, and here perhaps the best reply will be, to quote as it stands an utterance which I frankly admit to be beyond my powers of reply, for the plain reason that it is far beyond my powers of comprehension.

How came Hunter, he asks, to make his great discovery? And he answers thus:—

"The able and devoted assistants in his experiments and preparations well knew, and imparted that knowledge, when, in the course of my work descriptive of the Hunterian Physiological Collection, I found, besides the dry injected specimens, including that of the cured popliteal aneurism which Hunter obtained, long after the subsidence of the arterial tumour, on the death of his patient from another disease, that no fewer than twenty-four preparations (Nos. 163 to 187 inclusive) were defined in the scrap of MS. catalogue which Hunter had left respecting them, as exemplifying

the growth of the horns of deer. Could I doubt, on inquiring of his sole surviving pupil and assistant, the statement of the circumstances attendant on the experiments by which his collection was so enriched, and by which the experimenter was led to the discovery of a property of the arterial system previously unknown to and unsuspected by, physiologists?"

What was it, I would humbly ask, that Professor Owen could not doubt? And why could he not doubt it? By what new canon of "*omne minus in se continet majus*" was he led to conclude—if indeed he did conclude—that Hunter's cataloguing his specimens simply as illustrative of the growth of a stag's antlers proved him to have regarded them as the basis of a cure for a mortal human malady? By what peculiar process of arithmetic did he arrive at the conclusion that a series of twenty-four could possibly extend "from 163 to 187 inclusive?" And above all, what—or who—was the medium of communication with those "able and devoted assistants" of whom, he assures us, Mr. Clift was "the sole survivor?"*

3. Finally Mr. Owen resorts to that *ultima ratio* of our first childhood, that primæval polemic familiarly known as "calling names."

Into that line of controversy I must beg to be excused from following him. Every man must be of course the judge of what most accords with his own dignity. It seems to me that mine will be best consulted by leaving the venerable gentleman in this one respect master of the field.

Passing on to the matter more immediately in hand, Mr. Owen's second illustration of the fecundity of the vivisectional method of research, we seem to be met at

* This last is a point to which I would more particularly direct the attention of the Association for the Advancement of Medicine by Research.

the outset with a misapprehension as to the very nature of the discovery itself even more astounding in Harvey's case than the similar error in that of Hunter.

I say "seem," for, as I have already admitted, there are times when our author's utterances soar beyond me. Here at all events are his own words as communicated to the *British Medical Journal* of the 13th August last.

"This announcement of the 'lesser circulation unknown to physiology for more than a century after its record, became a lasting possession in human knowledge by Harvey's independent researches, in connection with that of the true nature and way of work of the whole cardio-vascular system—heart, veins, arteries of every part of the human frame—not merely the 'circulation of the blood,' but of its two-fold circulation, and this not by a new or different interpretation of structure, but by visible demonstrations of functions.'

Now surely if this mean anything—as I think one has fairly a right to assume it must, more or less—it means that the greater or systemic circulation being already a recognised fact Dr. Harvey supplemented it by his own original discovery of the lesser circulation through the lungs. Now this, though not precisely the reverse of fact is yet curiously near it.

In Harvey's time the pulmonary circulation was still no doubt to a great extent a subject of dispute and the first seven chapters of his great essay *De Motu Cordis*, &c., is taken up with the demonstration of this theory. And having completed this demonstration, he tells us that thus far he has "spoken of the passage of the blood from the veins into the arteries and of the manner in which it is transmitted and distributed by the action of the heart ; points to which some moved by the authority of Galen or Columbus or the reasoning of others will give their adhesion."*

Now these are not exactly the terms in which a man

* Harvey's Works, page 45.

speaks of his own altogether original invention. Nor again, when he tells us that :—“ From Galen, that great man, that father of physicians, it clearly appears that the blood passes through the lungs from the pulmonary artery into the minute branches of the pulmonary veins, urged to this both by the pulses of the heart and by the motions of the lungs and thorax.”*

But the lesser circulation thus disposed of he passes to his own discovery—the great discovery which so far from having been already demonstrated by Galen or Columbus is of “so novel and unheard of a character” that he not only “ fears injury to himself from the envy of a few, but trembles lest he has mankind at large for a detractor”—the grand discovery of the “greater or systemic circulation,” the passage of the blood not “from the veins to the arteries” but from the arteries to the veins.

It is not upon the authority of Galen or of Columbus that we are invited to accept our author’s conclusions now. The discoverer is speaking for himself and in the simplest and plainest phrase we learn how “ surveying his mass of evidence” and “long revolving in his mind” he at last “began to think whether there might not be a motion as it were in a circle.” And this, he placidly adds, “I afterwards found to be true.”

This then is the discovery with which we have to deal. Was it one which (1) as a matter of fact was made, and (2) as a matter of science could only have been made by means of vivisection? To both these questions I think I shall be able to supply, as in Hunter’s case, a direct negative. As in Hunter’s case also, I think I shall be able to prove even more than

* Harvey’s Works, page 44.

this—that this grand discovery not only need not have been made and was not made by these means but that it was physically impossible that it should be so made.

And here let me call attention to a fact which would seem singularly but constantly to escape the notice of all the numerous champions of Vivisection. They speak of Harvey's supposed vivisectional discovery as we in my youth of George Stephenson and the wonderful run from London to Birmingham in less than five hours. They seem to be under the impression that vivisection was a happy thought, hidden from the darkened mind of man till suddenly evolved by the brilliant boy of Folkestone, and then instantly revealing the great secret after which the mighty intellects of the past had, without it, been toiling in vain. They forget that it had been, certainly for 2,000 years, probably—according as we estimate our chronology—for twice or thrice or twenty times that period, the chief, as it was no doubt originally the only method of scientific research. If you want to know what goes on inside of anything, be it drum or be it thorax, cut it open and see; is the first instinctive "scientific" formula of the savage or the child. So far from having even developed since those earlier days, its practice had been narrowed and hampered to an extent to which the very small further restrictions since imposed are an addition scarcely appreciable. Harvey no doubt was free to cut up living dogs and cats without the necessity of first applying to an official inspector however friendly and accommodating. But the great truth so boldly proclaimed by Caiaphas and Sir W. Gull was as unrecognized in Harvey's time as now. That terrible foe of "Science," Christian civilization, had been doing its work even

then ; slowly enough but yet with some exactness, and the operations of the English vivisector were already restricted to "the lower animals." The Italy of Fallopius occupied much the same position in relation to England which the Italy of Mantegazza occupies now. Professor Lister, had he but had the happiness to live in those days, would not have cut short his late scientific trip at Lyons, and contented himself with "researches" in a few horses or mules. He would have gone on to Florence or Pisa, and got a Government grant of half-a-dozen human "criminals"—demagogues, heretics, indiscreet nuns, or what not—who would have been really worth the journey. A dozen or so of centuries further back still—with Jews a drug in the market, and Christians the natural sustenance of the menagerie, and the only vexed question as to the feeding of lampreys whether they fattened best on white slaves or black—the Professor Ferrier of the period would never have wasted his money on "highly intelligent monkeys," when at half the price he might try his red-hot wires upon the brains of as many learned Rabbis ; or Mr. Ernest Abraham Hart have devoted the pages of his *Journal* to records of crucified frogs when every sentence might be illustrated with cuts from the living organs of some "crysombd child." But by Harvey's time the English human subject was under protection—his hands were so far tied while his predecessors had been free.

Vivisection then had had it all its own way for, let us be moderate, and say 2,000 years. In whatever respects Harvey's practice of, or dependence on it differed from those of Galen or Hippocrates the difference was solely in that, in his case, the one was less free and the other less complete. I maintain—and my contention

is based as in Hunter's case, in the first place on the nature of the discovery itself, in the second upon the account of that discovery by the inventor—that it was precisely in these restrictions that his superiority lay. That he succeeded where others had failed, not because he was a greater vivisector than they, but because his clearer and more powerful intellect was less liable to be misled by the fallacies of that mistaken system, and less absolutely under its control. That in point of fact his discovery was made not by the aid of Vivisection but in spite of it.

Once more then—what are the facts?

The circulation of the blood is twofold. The lesser or pulmonary circulation, from the right ventricle of the heart, through the pulmonary artery to the lungs and back through the pulmonary vein and left auricle of the heart to the left ventricle; the greater or systemic circulation, from the left ventricle through the aorta and its branches into the capillaries and thence through the venous system, the vena cava and the right auricle back again to the right ventricle. The whole process has its analogy in the grander operations of the purification and fertilization of the soil. The ocean is the heart; the clouds the lungs; the showers the arterial; the rills, and brooks, and streams and rivers the venous system. Substitute for the showers a second riverine network, with its cloud-purified waters driven backward from river to brook, and brook to rill by the action of the sea, converted into a huge double force-pump, and performing mechanically the work now entrusted to successive evaporation and condensation, and the analogy becomes precise. Properly speaking the sequence here given is inverted; the pure and nutrient

blood starting from the left ventricle, depositing in its systemic course its fertilising ingredients and accumulating in their stead a mass of waste products; and in its pulmonary journey travelling to the lungs, there to be purified of its contaminations and re-impregnated with its vitalizing quality. I have taken them here not in their physiological but their historical order. In Harvey's time the lesser or pulmonary circulation was as we have seen a partially established fact. His grand work was the demonstration—in all but one final link at which to the last he could only guess—of the greater, or systemic.

And this work, even more plainly if possible than the work of Hunter in dealing with the failure of previous operations for aneurism, was essentially one of thought. It proceeded of course upon a basis of established but isolated facts, each and all of which individually, might possibly have been established, some of them even, let us grant, conceivably suggested, by Vivisection; but each and all of which, with one solitary exception, had already been established, most of them hundreds, some of them thousands of years before his time, by proofs wholly independent of any such corroboration. The impervious septum; the valves which at entrance and exit of auricle or ventricle intercept, here the ingress, there the egress or the regress of the blood-current; the jets which spirt from every severed artery in rhythmic cadence with each impulse of the heart; the invariable dilatation induced by compression, in every vein upon the distal, in every artery upon the cardiac side; the identity of characteristic between the arterial blood and that in the left ventricle and between the venous blood and that in the right

ventricle of the heart; the muscular structure of the heart itself, were each and all the fully established conditions of the problem with which he had to deal; had each and all been proved—were each and all in daily course of demonstration—in the routine experience of the operating theatre and the dissecting-room without the need of any vivisectional experiment of any kind. The one comparatively recent addition to this store of facts—a pregnant addition in its results on Harvey's mind though not really contributing to the argument very much more than had already been contributed by the valves of the right auricle—was the discovery by Harvey's own master, Fabricius of Aquapendente,* of the valves distributed throughout the veins. A discovery like the rest, purely of the dissecting-room.

And if the isolated conditions of the problem were thus effectively set forth without the aid of the vivisector's knife, its synthetic solution was a matter altogether beyond its reach. King Blood might, "go a progress through the veins and arteries of a beggar" and tokens of the regal presence might be manifested here and there. But to have the whole Royal route laid bare from end to end would be more than the vitality of the sturdiest mendicant could endure. Long before any given drop or teaspoonful of blood could have been traced—if indeed it conceivably ever could be traced—at the scalpel's point from ventricle to aorta, from aorta to iliac, from iliac to femoral, from femoral to popliteal, from popliteal to tibial and so back

* The claim of Fabricius to the merit of this discovery is (of course) disputed. Harvey himself speaks of him as the first who "gave a representation" of the valves in question.

in ever purpling course to vena cava and auricle once more, the most enduring colley that ever resigned his honest carcase to the vivisecting-trough would have tacitly abandoned his passive share in the investigation and left the investigator *planté là*, without any blood-current to trace.

And in perfect accordance with these very obvious facts is Harvey's own account of his own discovery. I do not pretend to say that like Hunter's history of the popliteal operation it proceeds altogether on the lines of philosophic argument without any trace of empiric experiment. Quite otherwise. The cruel fallacy which was the recreation of Hunter's scientific leisure, but the trammels of which his stronger temperament flung contemptuously aside the moment they began to hamper his practical work, was to Harvey the one only recognized road to knowledge. For him a physiological discovery could no more be accepted without some at least plausible vivisectional basis, than, when so provided, it could be submitted to his scientific colleagues in any less scientific language than that by aid of which their successors still invest with the wonder-working charm of mystery their simplest prescriptions of *Pil. Pan.* and *Aq. Pumpaginis*. He lays it down as a canon of scientific enquiry that "the senses" alone are the ultimate appeal and that though, when sensible demonstration is impossible the enquirer must needs be content with reason—"as he who enquires into the cause of an eclipse must be placed beyond the moon if he would ascertain it by sense"—yet that this gives very inferior results and "the example of astronomy is by no means to be followed." And so the conclusions of his clear insight and logical thought are resolutely held at arm's length till they can produce

at least some sort of witness from the senses.* To a Catholic sympathy, broad enough to embrace what it abhors and realise the human element even in inhumanity, there is something absolutely touching in the desperate perseverance with which he seeks to drag from his unresponsive fetich the revelation which his own clear brain had already afforded him. Like Baal's priests on Carmel, he cries aloud, and cuts "with knives and lancets"—not himself indeed but whole hecatombs of hapless animals of every grade. But there is no voice nor any that answers. Here and there perhaps a solitary experiment—like that for instance upon the heart of the snake—adds some slight corroboration to a fact thoroughly ascertained and proved before. But even these are either so deficient in the analogy of their original conditions or so gravely compromised by the disturbing influences of the experiments themselves, as to be practically almost as irrelevant as they are unquestionably superfluous. Let his great Essay be submitted to any impartial person, accustomed to deal with evi-

* The relation of Vivisection to true Physiological Science is very much that of Astrology to Astronomy. The prominence assumed in Harvey's great Essay by his vivisectinal efforts, and the amount of practical bearing exercised by them upon his argument, may be not inaptly illustrated by the journal of some scientific naval captain, who having from careful study of tides, currents, winds, &c., as recorded in the logs of previous voyagers, succeeded in mapping out a practicable North-West Passage, should feel bound to justify each day's work on orthodox astrological principles. The proportionate space occupied by the half-dozen logarithms from which at every noon his position on the chart is pricked, and by the sheets of hieroglyphics by which he would nightly demonstrate the oppositions of Saturn, favourable conjunctions of Jupiter and Venus and disturbing influences in the Houses of Mars, Bacchus, Apollo and Virorum by which it had been predicted, would be very much that of the argumentative to the experimental portion of the Essay. The relative value would be identical.

dence; to any judge or committee of judges, on the Bench; and let them decide whether in this matter Harvey learned any single thing from all the dogs he ever tortured—except perhaps the Latin in which he recounts the torments to which he subjected them.*

I will go even further than this.

It is the fashion to speak of Harvey's great treatise as a model of plainness and lucidity. I venture to think that those who thus speak, speak from hearsay and have never read it for themselves. Full of clear thought and cogent argument it is, and couched every here and there in language as clear and as cogent as either. But only here and there. The general effect is involved and confused. So confused and so involved that strange as it may seem neither our Professor nor Harvey's own translator have been able to carry away a clear idea of his meaning.

In the inevitable enthusiasm of the biographer, for example, Dr. Willis not only maintains with, I think perfect justice, Harvey's claim to the honour of independent discovery without any suggestion from the previous but unpublished work of Servetus but valiantly denies that Servetus made any discovery at all.† But whilst thus bent

* And which seems sometimes to have puzzled even his translator. Take for example our author's statement ". . . è cavitate pectoris . . . pus . . . per arterias, cum urinâ vel cum fæcibus alvi, posse expelli." Which the Sydenham Society "traduces" into the startling assurance that *all three of the superfluities in question can be conveniently discharged through the arteries!* —Page 18.

† As a matter of fact Servetus not only does describe the pulmonary circulation in the plainest terms, but on one important point is right where Harvey is wrong. The effete blood says the latter "returns to its sovereign the heart . . . there to recover its state

on unduly exalting his hero at the expense of Servetus, he curiously fails of doing him bare justice in the case of Malpighi. Harvey, he tells us "had no notion of the one order of sanguiferous vessels ending by uninterrupted continuity or by an intermediate vascular network, in the other order."* Yet, speaking of the passage of the blood from artery to vein at the extremity of a limb, Harvey not only suggests in so many words "an anastomosis of the two orders of vessels" as an alternative to the assumption of "pores in the flesh . . . permeable to the blood,"† but puts the former suggestion first, not inconceivably, I think, as being in his eyes the more probable.

Mr. Owen's misreading of his author is more striking still. "Harvey" he tells us "showed his hearers and watchers that the organ became erect, and gave the beat which we feel upon the chest; then that it contracted, became notably shorter and narrower."

Now the "movement of the heart" was precisely the one thing which Harvey's vivisections really did, not indeed suggest but demonstrate. And the demonstration was precisely the reverse of that which the apostle of Vivisection here attributes to it. The notion that, as Mr. Owen tells us, it is the diastole or expansion of the heart which gives the beat and that the beat thus given is followed by its systole or contraction is precisely the popular error which Harvey sets himself to refute. "It is generally believed," he reminds us, "that when the heart strikes the breast . . . the heart is dilated . . . but the contrary of this is the fact, and

of excellence or perfection. Here it resumes its due fluidity . . . and is impregnated with spirits," &c. "*In the lungs*" says Servetus on the other hand "does the mixture take place."

* Page xli.

† Page 58.

the heart *when it contracts and the shock is given*, is emptied.”*

Of course I do not for a moment pretend to say that, as concerns the actual physiological fact, Mr. Owen is not right and Harvey wrong. When two such doctors disagree so flatly, it is not for a mere outsider like myself to take upon him the decision. Nor is the decision one in which, from this particular point of view, I take any very special interest. My present concern is not with the result of the inquiry but with its method and here the bearing of this curious little discrepancy is the same in either event. Whether it were the immortal Harvey who bungled his experiment or the venerable Owen who blundered in his report, the pit into which either giant fell was equally dug for him by Vivisection. To Vivisection accordingly I am enabled with much gratification to attribute at length this one practical result; a result significant at all events, even if not strictly satisfactory.

Significant more especially, I venture to think, of its general influence on the whole course of the argument. We will not waste time on admittedly unsatisfactory experiments. Let us take, as crucial tests of the futility or otherwise of this mode of investigation the two experiments expressly recorded by Harvey as having almost alone furnished a thoroughly successful result.

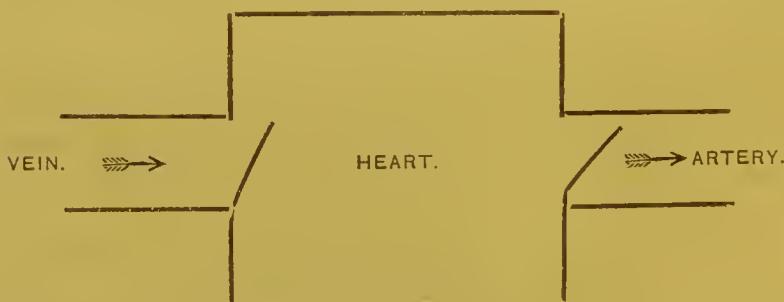
So bewildering, he tells us at starting, were the movements of the heart as studied under the somewhat abnormal conditions of a dog’s thorax violently broken open, with flayed chest and ribs notched and snapped off one by one for the purpose, that he “began to think with Frascatorius that the actions of

* Page 22.

the heart were only to be understood by God." Only in the last few faint beats could he trace any distinct sequence at all. Fortunately the sequence of movements so traced agreed with his preconceived and perfectly accurate views and Harvey was happy.

Suppose that instead of agreeing with his views these same observations had been diametrically opposed to them? Need his happiness have been a whit the less? Not at all. The explanation would have been simple. "This special sequence of movement," we should then have been told, "is observed only immediately before death, which follows it as a matter of course. It is thus abundantly clear that, even if this particular sequence of movement be not the direct cause it is the invariable precursor of death and must therefore presumably differ essentially from those which are the support of life. Q. E. D."—as before.

The heart of a snake, on the other hand, is a structure very closely resembling the simple india-rubber apparatus used for the production of ether-spray and for other purposes. A central blood cistern—of which the following rough diagram may perhaps serve as a



tolerably intelligible illustration—has two openings, the one into the principal artery the other out of the principal vein. Across the former of these is

placed a valve admitting of free egress but cutting off all return. Across the latter is a similar apparatus acting in the inverse sense, permitting ingress and forbidding exit. The great vivisector's experiment clearly proved that when you tightly pinched the artery by which alone, owing to the valve at the mouth of the vein, the blood could escape,—the blood could not escape any longer. But that surely was not all? No. Not quite. Releasing the artery through which the blood runs out he applied a similar compression to the vein, by which alone, owing to the valve at the mouth of the artery, it could possibly run in. And then he found—that it did not run in!* And was as much delighted with the results of this "scientific" procedure as though he had clearly proved, by building a wall across the passage, that he could not get in to dinner when the dining-room door was shut.

Nor is this by any means the only instance of the kind. The majority of the so-called experiments are not strictly speaking experiments at all; mere repetitions for corroborative or elucidative purposes of the ordinary experiences of every day surgery.†

Let me give one more the result of which is simple error and in which the error is more unmistakeably with the experimenter and its origin even more clearly

* Page 54.

† As for instance with the famous exhibition at Court where he narrates how he dissected out and cut the internal jugular for the sake of showing that while but a few drops of blood escaped from the lesser orifice "a round torrent of blood" gushed down from the head; he quietly concludes his story with the remark—"You may observe the same any day in practising phlebotomy."—Page 126.

and directly traceable to the method by which he worked.

"Having," says Harvey, "exposed an artery and divided it so that the blood shall flow out as fast and as freely as it is received you will scarcely perceive any pulse in that vessel. . . . An artery denuded and divided in the way I have indicated sustains no shock and therefore does not pulsate. *Whence it clearly appears that the arteries have no inherent pulsative power and that neither do they derive any from the heart.*"*

Now here we have the experimental method in its ideal form; experiment used not merely to illustrate conclusions previously arrived at by mental process but as basis for a conclusion of its own. And the conclusion is wrong.

Compare the simple cogency of the following piece of plain reasoning from the plain anatomical facts of the capacity of the left ventricle and the amount of blood it must as a mechanical necessity, expel at each contraction. "Let us assume" he says:—

"The quantity of blood which the left ventricle of the heart will contain when distended to be, say two ounces, three ounces, one ounce and a half—in the dead body I have found it to hold upwards of two ounces. Let us assume further how much less the heart will hold in the contracted than in the dilated state and how much blood it will project into the aorta upon each contraction—and all the world knows that with the systole something is always projected, a necessary consequence demonstrated in the third class, and obvious from the structure of the valves. And let us suppose as approaching the truth that the fourth or fifth or sixth or even the eighth part of its charge is thrown into the artery at each contraction. This would give either half-an-ounce or three

* Page 18.

drachms, or one drachm of blood as propelled by the heart at each pulse into the aorta. Which quantity, by reason of the valves at the root of the vessel can by no means return into the ventricle. Now in the course of half-an-hour the heart will have made more than one thousand beats. Multiplying number of drachms propelled by the number of pulses we shall have either one thousand half ounces or one thousand times three drachms or a like proportional quantity of blood according to the amount which we assume as propelled with each stroke of the heart, sent from this organ into the artery. A larger quantity in every case than is contained in the whole body!**

And this as a matter of fact is, according to Harvey's own account the argument on which his great discovery is based and out of which it sprung. And a most clear cogent and unanswerable argument it is. The brilliant because simple synthesis of the long string of plain anatomical facts relating to the structure and capacity of heart, artery and vein. A piece of pure reasoning with which the experimental method of Vivisection has—happily—no more to do than the rites of Obeah or of Shaman.

I have already once before alluded to that dramatic scene on Carmel. Let me, in all reverence, draw from it now not an illustration merely but a suggestion. Let us divide between us Harvey's great treatise as the rival prophets of JEHOVAH and of Baal divided the bullocks of the decisive sacrifice. To you, gentlemen, as to the more numerous body I, like the solitary Tishbite, yield the choice. Take you the whole group of vivisectional experiments appealed to by the discoverer from his first page to his last. I will content myself with what is left—the plain and simple reasoning which could have been equally adduced, and would have been of equal cogency, had Mr. Reid's Bill been already

* Page 48.

passed, and the whole Executive of the Victoria Street Society present to enforce it. Then let the two be laid before any unbiassed arbiter trained to estimate the weight of evidence, and let him say from which of these two "methods of research" the great discovery sprang.

Finally, I may be told that the only discoverer is he who demonstrates. I accept the definition.

How then is this discovery, made as we have seen without the aid of Vivisection, to be most effectively demonstrated? Not certainly by experiment upon living animals. The essential, self-evident impossibility of any such demonstration has already been clearly shown. Is it needful that to you gentlemen, practically familiar as most, at all events, of you must be with the every-day routine of the hospital dissecting-room I should suggest a method which surely must suggest itself to anyone not absolutely a slave to the vicious tradition of Vivisection? You have but to take the first "subject" that comes to hand in any of the great teaching establishments at your disposal, and with the syringe which the porter will promptly furnish inject the aorta with matter of any colour or any series of colours you may prefer until the injection issue from the vena cava, and the thing is done.

Solvitur circulando.

One word in conclusion. I trust that nothing I have here said of Harvey or of his discovery will be construed into an indication of anything but sincere respect alike for the discovery and the discoverer. Having unfortunately—I would have avoided it if possible—

been compelled to mention the name of Servetus I am of course prepared to hear my present argument described as a 'simple assertion of plagiarism from him just as my former one was set down on similar grounds as taxing Hunter with copying Anel. It is not with any idea of preventing that assertion but simply to save time in refuting it that I conclude as I began, with a plain statement of what my views on that head really are. I think it most probable, then, that Harvey never saw or heard of that curious lucubration in the nebulous recesses of whose misty mysticism Michael Servetus has so oddly encysted the eminently practical deduction from his earlier experiences of the dissecting-room. I am very sure that, had it lain upon his study table from that day to this he would no more have been tempted to read it than my old friend Mr. Norman Lockyer* will be tempted to seek for information on the chemical constituents of the sun from the pages of the "New Dispensation." As practical possessions of medical science the lesser circulation is mainly, the greater entirely, the work of William Harvey.

And rarely indeed do we encounter a discoverer to whom the fullest credit of his work can be accorded with more ungrudging heartiness. It was no fault of his if, while his intellectual powers soared far beyond the ablest of his compeers his ethical standpoint yet remained, in some respects at least, only at their level. Two centuries and a half ago the moral responsibilities of power towards weakness were but loosely formulated as between man and man. The idea of claims on the part of any inferior class was an unrealised potentiality even of the Christian code.

* It may be as well perhaps to mention here that I am not accusing Harvey of plagiarising from Mr. Norman Lockyer.

And here Harvey was simply of his time. He cut open a dog with no more thought of cruelty and probably much less consciousness of wrong than, as a schoolboy he might have cut open his sister's doll. And the fault was the fault of his time. It was not for the brutality of his vivisections that his contemporary critics blamed him but for their "vain glory." And in all else—in singleness of purpose, in modesty of self-estimate, in moderation of tone even towards opponents whose claims to it at his hands were very small he sets to controversialists of every class an example which, in the last particular at least I confess that I for one find it sometimes easier to appreciate than to follow.

May I hope that in this last particular I shall find it followed by that great Association to whose Presidents and Council I have ventured most respectfully to address this humble contribution towards the advancement of medicine by research?
